ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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VOLUME XXX

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

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THE FURTHER PROCEEDINGS OF THIS INQUIRY RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Can we now come to order?

MR. LASKIN: I think we can, Mr. Chairman.

DR. DUPRE: Thank you for returning, Dr. Finkelstein, and welcome back.

I assume that we can consider the witness already sworn in and he is not in need of a booster shot or another injection?

MR. LASKIN: I think that's fair.

DR. DUPRE: Okay, do you have any matters to raise before we proceed, counsel?

MR. LASKIN: I don't, Mr. Chairman.

DR. DUPRE: Do the parties have matters to raise?

MR. LASKIN: M. Casgrain, I understand, will be a little late, but I think we should proceed.

DR. DUPRE: Fine.

As I recall, Mr. Hardy, you were engaged in the process of cross-examination when we last adjourned Dr.

DR. DUPRE: (cont'd.) Finkelstein, so will you proceed, please, sir?

MR. HARDY: Yes. Thank you, Mr. Chairman.

DR. MURRAY FINKELSTEIN, PREVIOUSLY SWORN, RESUMES THE WITNESS STAND
CROSS-EXAMINATION BY MR. HARDY, CONTINUED

Q. We had a good bit of discussion about the exposures of the men in your two studies, Dr. Finkelstein, and I would just like to clear up a few things on the exposures.

I believe when you were presenting your studies on the prior day, you made some reference to the amount of crocidolite used in this plant, and I believe you used the figure three percent, but I don't believe it was clear exactly what you were referring to.

Maybe I could just let you describe the extent to which crocidolite, as opposed to chrysotile, is used in the plant over the years?

A. I don't recall using the figure three percent, although apparently in the transite pipe an average figure for crocidolite was three percent by weight.

Q. That's three percent of the total pipe weight?

A. I'll have to pass on that. I'm not sure whether it's three percent of the twenty-five percent of the pipe which was asbestos, or three percent of the total pipe by weight.

However, the proportions varied all depending on the diameter of the pipe that was being made. The larger diameter of pipe required more crocidolite than the smaller diameter pipe, so that's sort of a ballpark figure anyway.

Crocidolite was not used in the board shop, so the pipe shop was the only area of the plant where crocidolite was used.

Q. Do you have any data on which of the men in either of your studies were exposed or were not exposed to

Q. (cont'd.) crocidolite?

A. I know where each of the men worked. I have presumed that everyone who worked in the pipe shop was exposed to crocidolite. I also presumed that the maintenance men, who must have passed through that shop as part of their work, were exposed to crocidolite as well.

Q. You have not to this point displayed any of the morbidity and mortality results in terms of crocidolite, chrysotile or the exposure, I gather?

A. No. As opposed to what Weill and Enterline can do, where presumably crocidolite and chrysotile were used simultaneously over the whole history of the plant, the flex board shop at Scarborough only opened up in July of 1955, so men who would have had a pure chrysotile exposure were first exposed at least seven years after those who started working in the other plant, and because of the latent interval considerations it is not yet profitable to attempt to separate out the group who worked only in the flex board plant.

So five or ten years down the road it may be reasonable to split them into two groups. At this point in time it is not a worthwhile thing to try.

Q. Do I gather correctly from that description of when the flex board shop began that almost all the men in both your morbidity and mortality cohorts would have been exposed to crocidolite?

A. I can't give you a proportion. Most of them probably were. But when the flex board plant opened, there was a lot of fresh hiring and there were certainly a fair number of men who came in to work specifically in that area of the plant.

I cannot give you at this point a breakdown as to the figures.

Q. As I understand the cohorts in your mortality

Q. (cont'd.) study, the average age of the men in all three of the cohorts, including the group who were not exposed while working for Johns-Manville, to asbestos, was thirty-three years?

A. About that.

Q. I would guess that most of these men, therefore, had at least ten years, maybe longer, of working experience prior to beginning work at Johns-Manville?

A. Many of them worked elsewhere, yes.

Q. You don't have any particular information on exposures that any of these men might have had in those ten-plus years?

A. Everyone who was seen by the Compensation Board had an occupational history taken by the Compensation Board, so the hundred-odd men who have been compensated by the Board do have complete occupational histories.

Looking at those, virtually no one would have had asbestos exposure prior to starting at the plant. Which is not to say that the men who haven't been compensated and haven't had these complete occupational histories didn't, but if they were all representative, I can think of one man who may have worked in a shipyard in Halifax before coming to Ontario.

Q. You just referred to a hundred men who were compensated?

A. That's global figures.

Q. That's the global figure for what group?

A. Johns-Manville employees.

Q. That's all Johns-Manville employees who have been compensated by the Workmen's Compensation Board?

A. Yes, for whatever cause. Yes.

Q. Now, that hundred men overlaps, I would guess, to some extent with your mortality cohort, but it doesn't overlap completely?

A. Yes, that's right.

Q. Do you have any idea how many of those hundred men for whom you have looked at complete employment records are in the three hundred and thirty-nine men, I think it is, in your mortality cohort?

A. What I can tell you is, the complete employment histories I have looked at, I think I spotted one man who may have had an exposure in a shipyard. I think there was another man who may have been employed in Quebec before coming to Ontario, and that was about it.

So, you know, if these men are at all representative of the men who didn't have complete employment histories taken by the Compensation Board, then virtually no one employed at Scarborough would have been exposed to asbestos earlier.

Q. I wonder whether that generalization would hold up in light of the interesting contrast which, if we could turn to table two of your mortality study, where you show that the age of first exposure of the nineteen men in that cohort who died of lung cancer was thirty-nine years, which is first of all higher than the average age of the cohort and also considerably higher than the average age of the men who died of mesothelioma?

A. Yes.

Q. Do you have any explanation for this unusually high age for the lung cancer victims, as a first exposure?

A. I think it's probably a question of biology. I think that, you know, if the model that I presented and the model that Peto discussed is correct, then the effect of asbestos exposure serves to multiply the underlying population lung cancer risk. The population lung cancer risk is only substantial in older men. Therefore, it's only, you know, given

A. (cont'd.) that the plant was only in operation twenty or thirty years, it's only those men who were somewhat older when they started who would have entered the region where the underlying lung cancer risk is substantial, and these are the men whose risk would have been multiplied by their asbestos exposure and would have died of lung cancer.

So I think it's, you know, probably a biological phenomenon rather than an employment phenomenon.

You know, the model that I proposed is that mesothelioma is not age dependent and it's purely a latent kind of phenomenon, in which case the younger men would be more expected to develop mesotheliomas, and in lung cancer where it's a multiplicative kind of thing it would be the older men who would be dying of lung cancer.

I view this as biology rather than employment.

Q. In referring, as you just have, to an underlying lung cancer rate being fairly substantial, are you therefore referring primarily to smokers?

A. I'm referring to the general population, which is a mix of smokers and nonsmokers. Certainly the lung cancer risk in smokers is much, much larger than in nonsmokers.

Q. You have presented us with a table on the mesothelioma deaths among workers who you looked at at this plant. I guess it's tab seven of exhibit thirty-six. I wanted to clarify the reference on that table in the final column under pathology, to the CTRC - Canadian Tumor Reference Center. Could you tell us what that means?

A. Okay. Before I get to that, I might add that since I last saw you there is an addition to this table. This is a man which would be case sixteen, who was employed from 1955 to 1958, died in June of 1981 at the age of fifty-three.

This was a pleural mesothelioma. He has been

A. (cont'd.) autopsied, but the pathology has not yet been reviewed.

MR. LASKIN: Worked from when?

THE WITNESS: From 1955 to 1958...in the pipe plant.

The National Cancer Institute in Ottawa has set up a Tumor Reference Center which is housed at the Ottawa Civic Hospital. The director of the Institute is a Dr. McCawhee, who is one of the world's foremost experts on the pathology of mesothelioma.

I contacted him, he agreed to review the material for all those cases in which it was possible to obtain either biopsy or autopsy material, and it's actually...there is a mesothelioma panel. I guess this is the Canadian Mesothelioma Panel, one member of which was actually an American, Dr. Kannerstein, who I think was a member of the American Mesothelioma Panel as well.

So some of this material has been reviewed only by Dr. McCawhee and one of his colleagues in Ottawa, and some of it has been sent out to the entire Canadian Tumor Reference...or Mesothelioma Panel.

This is the same group that was used by the McDonalds from McGill when they had their mesothelioma cases reviewed.

Q. Right. That's what I was curious about - to what extent CTRC meant that there was panel review as opposed to an individual review, and I guess what you are telling me is there was some of both?

A. Yeah. In one or two cases Dr. McCawhee would look at the slide, said this was such a classic case that unless I felt, it would not have to be absolutely rigorous and have the material reviewed by several other people, that he felt it was so clear cut

A. (cont'd.) that there was no point in having further review.

But many of the cases were reviewed by the panel.

Q. Then do I recall correctly that none of these referred cases were concluded to have been misdiagnosed as mesothelioma by the Canadian Tumor Reference Center?

A. There was one misdiagnosis, but it's not on this list. It was one of the cases that I referred to which resulted in the revision of the tables in the mortality table.

All the others have been confirmed.

Q. Is it your intention to have the mesotheliomas that haven't yet been submitted to the Tumor Reference Center submitted, or is Professor Ritchie, in your mind, a suitable substitute?

A. Yes, I think so.

Q. Therefore you are not going to have the ones that are listed as Professor Ritchie submitted for further review?

A. No.

Professor Ritchie, I might add, is the Compensation Board's pathology consultant. He is the man who examines the pathology in the cases that come before the Board.

Q. Was there any criteria that you used in deciding which of the mesothelioma cases should be referred to Ottawa to the National Cancer Institute, and which should not?

A. The cases that Dr. Ritchie had seen, I decided I would accept and not send on. The ones that he hadn't reviewed, I sent to Ottawa.

Q. I'm curious about the differential you found between observed and expected with respect to lung cancer, in the mortality study. First of all, maybe if we look at table one, as I understand your results you find an observed-to-expected ratio for lung cancer of about eight?

A. Yes.

Q. Which shows up, if we follow the lung cancer line all the way across, as the final figure on the right on table one.

And the eight comes from comparing the production and maintenance workers nineteen observed lung cancers to an expected number of two point five?

A. Yes.

Q. Could you explain where the two point five number comes from?

A. Two point five is based on a man-years calculation. You multiply the person years at risk by the population mortality rate, and you come up with a product which is the expected number of deaths.

Q. What did you use for the population mortality rate?

A. I used the Ontario calendars, age in calendars specific mortality rates published by Statistics Canada.

Q. For any particular period?

A. For whatever relevant period...they are published in five year intervals. I quoted in one of the tables 1970/1974 rates. That was just for illustrative purposes. But for the period 1966 to 1970, I used those rates for 1970 to 1975. I used those, and for 1975 to 1980, or whatever. I think they are centered around census years, which are odd years, so it's 1971 to 1977, or something like that.

Q. So what you are saying is that for this particular calculation, to determine the two point five expected, you used the varying rates over time...?

A. Yes.

Q. ...applied to this cohort, which also was living over time?

A. Yes.

Q. You used the rates for Ontario?

A. Yes.

Q. In deciding to use the rates for the entire province of Ontario, did you also consider the possibility of using the rates for Metropolitan Toronto?

A. Not very strongly. The larger your population base, the more stable the rates because there's random fluctuation in any given year so that those rates were available, they were more stable than the Toronto rates, so we decided to use them.

Q. Do you know, by chance, how the Metropolitan Toronto rates compare with the entire provincial rates?

A. I don't, no.

Q. I guess the reason I was particularly curious as to whether you knew the Toronto rates was that it occurred to me that the two point five expected number is a fairly small number, and...

A. I might point out that many of these men died outside of Metropolitan Toronto - Pickering, Oshawa...outlying communities which are not in Metropolitan Toronto.

Q. They all worked in Metropolitan Toronto?

A. I believe actually that West Hill is outside the boundary of Metropolitan Toronto. I'm not a geography, so I can't attest to that, but I believe you pass the Scarborough boundary on the way to the plant from this direction.

Q. The reason I was curious about whether the rates would make any difference is that two point five is a fairly small number and if it were not much larger, say four or five, the observed-to-expected ratio would change considerably and you might be talking about a relative risk of four rather than eight, so I wondered whether there was a possibility that had you used a different underlying rate to determined expected, it might have

Q. (cont'd.) dramatic effects on the relative rate?

A. It may have effects of the order of ten to twenty percent. It certainly wouldn't have a doubling effect.

Q. But you didn't...you are not sure what the Toronto rates are?

A. No, I'm not.

Q. You suspect that they wouldn't be more than...

A. I'm not sure, but I would be absolutely shocked if they were a factor of two higher.

Q. The one other thing I wanted to ask you about the expected number is that...no, I'll come back to that.

Maybe we could talk for a minute about your mortality results as you discuss them in the paper, and as I see it you discuss results for mesothelioma, gastrointestinal cancer and lung cancer?

A. Yes.

Q. Why don't we first talk about what you find with respect to mesothelioma in the cohort. Am I reading you correctly on, I guess it's page fifteen, when you indicate that your findings are roughly comparable to the mortality rates for mesothelioma found in English textile factory study?

A. Well, I was just fishing around for something to compare with and this was the only figure in the literature that I was able to compare one of the my three exposure groups with one of the groups in the English mortality study, and they were roughly comparable. But I wouldn't put especially much cogence on that.

It was in a sense reassuring and, you know, my results were in the same ballpark as what they found in England. But I think a ballpark comparison is about as good as one would hope to do in this situation.

Q. I gather for gastrointestinal cancer your conclusion is that your study has too few deaths of that type to

Q. (cont'd.) draw any definitive conclusions?

A. Yes, that's true. The only suggested thing is that the mortality rates appear to increase with dose, which is consistent with the hypothesis that there is a cause-and-effect relationship, but it certainly doesn't prove that.

With the small size of my study it would be possible to obtain statistical significance only with increased risks which are of the order of two to four times elevated above background.

If lung cancer risks were...or gastrointestinal cancer risks were only elevated by fifty percent or a hundred percent - which is still quite a sizable increase in risk - the numbers of men in my study were so small that I just couldn't detect that.

Q. With respect to lung cancer, I gather that though you find an increased risk of lung cancer in the cohort, at the same time you don't find a very good or in fact any dose-response relationship for lung cancers, and I guess that can be seen from table four?

A. Well, that was my conclusion. If I could...well, it doesn't conform to the nice pattern I would hope to see. If I made the assumption that my last point was wrong, then the first two, including the zero exposure rate, fall in a nice straight line, but certainly the third point is way out of line with the others. So my conclusion is the same as Peto's in this particular circumstance that there was no mathematically pretty relationship that I could ascribe to the workers in this plant.

Q. You referred to your third point, and maybe we should clarify, by looking at table four, what you mean by the third point. Perhaps you could do that for us?

A. That's group C.

Q. In other words, the rate per thousand man years for lung cancer found in group C, which is the third of the production

Q. (cont'd.) workers who were most heavily exposed, is nine point nine, according to table four?

A. It's eleven point nine on the corrected table. There is one additional lung cancer death which is originally attributed to mesothelioma.

Q. Do we have a corrected table four?

Okay, eleven point nine on the corrected table four. In referring to that figure as not what would have been expected or not what you would have liked, what you are referring to is the fact that that number is considerably lower than the twenty-six point one rate for group B, which is the middle-exposed group, and in fact even lower than group A, which is the third of your workers least exposed?

A. Yes, that's right.

Q. I gather that despite the fact that you didn't find a dose-response relationship for lung cancer in this group, you nonetheless attempted to do some extrapolation of lung cancer risks based on your study?

A. What do you mean by extrapolation?

Q. Well, you made some predictions in the addendum to the mortality study. I guess it's called an assessment of risk.

A. Yes, I assumed a linear exposure-response relationship, which is standard in this kind of work, and made use of group averages. This is exactly what Peto did in his study.

Q. You say you made use of group averages. By that I gather what you took is what you calculated to be the average exposure...

A. Yes.

Q. ...for all workers in the cohort?

A. Yes.

Q. And considered the number of lung...or the relative risk of lung cancer for all workers, and using those two

Q. (cont'd.) factors and assuming linearity, extrapolated risks at other exposure levels?

A. Yes.

Q. I gather then that the dose-response relationships, or lack thereof, found in your actual look at the cohort would give you numbers elsewhere other than on this linear curve you assumed for use of the risk assessment?

A. Can you rephrase that?

Q. I gather that the actual dose-response relationship found in the data when you divided it into three groups would be different than the dose-response relationship you assumed for purposes of assessing risk?

A. Well, I concluded that I couldn't make any useful conclusions as to what the exposure-response relationship would be, and so with the mesothelioma where it appeared to be linear, I calculated the exposure-response in two ways - using the curve from the Johns-Manville, the A B C group and using the group averages in the second approach.

For the lung cancers, I didn't think the first approach was feasible because of the lack of a nice mathematical relationship, so I was forced to use the linearity assumption and the group average.

Q. In constructing the risk assessment, without going into the details of how you did it, you present results on pages seventeen, eighteen and nineteen of your mortality study, and you present results in two ways, both for the mesothelioma and for the mesothelioma plus lung cancer predictions.

A. Yes.

Q. One way is to present the results assuming the accuracy of your exposure calculations, is that right?

A. No. The first way is to use the slope of the

A. (cont'd.) exposure-response curve for the mesothelioma. The second way is to use the group average approach.

Q. Okay. But then beyond that you first report what the linear dose-response model would predict for a man who worked in a concentration of one fiber per c.c. for, I believe it's eighteen years, if your exposure calculations are correct?

A. Yes.

Q. Then you also report what that same result would be if your exposure calculations were off by a factor of ten?

A. Yes.

Q. I gather that the results, if your exposure calculations are off by a factor of ten, as reported on page nineteen, would be that, at least for lung cancer and mesothelioma, a total of sixteen out of a thousand men might occur, compared to twelve to thirteen expected?

Or in other words, an excess of three out of a thousand?

A. Between the ages of forty-five and sixty, yes.

Q. Then your final conclusion, at the bottom of page nineteen, is that because of the large uncertainties of exposure it's impossible to assess the risk with any accuracy?

A. Yes, I think that's true.

Q. I gather that's why you give the wide range of the figures both with your calculated exposures and what it would be if the exposures were ten times as great as you calculated?

A. That's right.

Q. You made some general references at the end of your testimony about, I believe you called them options for future actions?

A. Yes.

Q. I'm sort of curious to expand on your views in that regard. Perhaps you could explain for us what you meant by

Q. (cont'd.) your phrase on that page, which is the last page of tab seven, is that correct? Where you say that:

"The potential for damage in psychological trauma is real, and any action must be evaluated for its possible harmful impact".

Could you give us some idea of what sort of action you are talking about that might have harmful impact?

A. Well, I think that any action you would care to propose, no matter what it may be, could in principle have some harmful impact and should be examined in that light.

Like, off the top of my head, I could name two things - a sputum cytology program or announcements in the newspapers that former workers should visit their doctors every three weeks for an x-ray because they may die of lung cancer tomorrow. Those are two actions which are conceivable, which could have a harmful impact.

There are hundreds of other conceivable actions and any one you would care to propose should be examined in that light.

Q. Well, I'm curious about one of those you mentioned, which is the announcements over the media to see your doctor, since there was a great deal of that done in the United States in the last five years and I would be curious to get your view of whether those sorts of announcements cause undue psychological trauma?

A. I haven't talked to anyone living in California to find out how they felt about it. It would not surprise me at all if people were frightened by these announcements, and the benefit of the announcement could quite conceivably be considerably less than the lack of benefit produced by fright.

I wouldn't like to see the same sort of action in Ontario.

Q. It would be your view that people would hear

Q. (cont'd.) those announcements...maybe you should explain...you indicated would not get any benefit from them. Maybe you should expand on that.

A. I didn't say that.

Q. You said less benefit than harm. Maybe you should explain what the benefit is intended to be and why you doubt that it would be...

A. I really wouldn't care to comment on what went on in the States. If you would like to ask me about what I think we should do here, I think I can be more specific.

Q. I gather though that you don't see any overall benefit from widespread notification programs to workers that they might have worked with asbestos in the past?

A. I think there is good reason to look at special cases. I think Johns-Manville workers are one special group. I think wholesale announcements that anyone who has ever come into contact with an asbestos fiber should immediately see their physician is what could be very damaging.

Q. In talking about actions that might have a possible harmful impact, would you include in that warnings or widespread announcements about possible consequences of asbestos in schools?

A. I'm not quite sure what I should say about that. I have an opinion which may be somewhat in conflict with the official position.

Q. I think it was clear at the beginning of your testimony that you were here to present your own personal views, if there is any problem with misinterpretation.

A. My feeling is that the risk to people working in schools, exposed to whatever ambient levels of asbestos there are in the schools, is probably inconsequential. As part of my work I am occasionally called upon to field telephone calls from

A. (cont'd.) worried people who read the newspapers and listen to the radio, and I think this is possibly one instance where people have been worried...the harm done by the psychological trauma has possibly been greater than the benefit derived in cleaning up the asbestos in the schools.

Q. I gather you make that comment as a person who is familiar with the risks of asbestos at high levels in occupational contacts, and who has attempted to extrapolate those risks to lower levels, at least in the occupational setting?

A. I am convinced that there is a gradient risk. The greatest risk is faced by people who work in environments where they had a hard time seeing their friends across the hall. I am convinced that the risk to an individual who may inhale an asbestos fiber happens to be floating by is orders of magnitude less than the risk faced by the occupationally-exposed people, and you know, I think Dr. Enterline made a very good point when he said we have only so many resources to expend in our society and one has to rank problems in order of importance. I think that ambient asbestos exposures are probably less of a public health problem than other things.

MR. HARDY: I have no further quetions, Mr. Chairman.

DR. DUPRE: Thank you, counsel.

Next?

Miss Jolley, please.

CROSS-EXAMINATION BY MISS JOLLEY

Q. You would, however, indicate that perhaps occupational exposures are a major problem?

A. They certainly have been in the past. It remains to be seen whether or not there will be a problem with exposures at current levels.

Q. In terms of the public buildings, would you say the risk to maintenance personnel was a significant risk?

A. You know, I think this has to be looked at on an individual basis. I don't think one can make the blanket statement that maintenance personnel are at risk. Certainly someone who spends their entire work week stripping lagging off pipes is quite conceivably at risk.

Q. You did, however, in your review of the Workmen's Compensation, find that in fact there were maintenance personnel compensated in the province?

A. I believe so.

Q. In the school boards?

A. Again, I don't know what these individuals...how they spent their working lives.

Q. The report of the U.S. experience, is that what you are talking about when you say avoid and discourage hysterical announcements?

A. I think some announcements closer to home would fall into that category as well.

Q. What would those be?

A. Actually, I retract that. I was thinking more of the comments further on in that line.

Q. Perhaps also would education not only discourage smoking but inform workers fully of the hazards connected with asbestos?

A. Oh, I'm thoroughly convinced that workers should be educated about the materials they are working with. Proposed occupational regulations for toxic substances encourages education of the work force.

Q. The other part of that that I would like to ask a question is, the whole surveillance issue, and I think that I agree with you in terms of the medical monitoring, but I wonder

Q. (cont'd.) whether it isn't useful to have a registry of asbestos workers?

A. Could you indicate how you think this would be useful?

Q. Well, I think for future compensation purposes. For instance would it not be useful to know...and also for epidemiological purposes...to know who worked at Johns-Manville? Or for instance, how many of the workers in Ontario this summer are ripping out asbestos, and who they are?

A. The problem is, I'm not sure what you would do with that information. Given that ten percent of Ontario men will die of lung cancer, if you've got...it would seem unreasonable to compensate everyone on this list who dies of lung cancer just because they happened to be on the list.

Similarly, I don't think that very much useful epidemiology could be done with a list like that. We know that exposure to asbestos causes all these diseases. We would be reinventing the wheel by looking at a list and discovering that there were perhaps more lung cancers and mesotheliomas, or whatever.

Q. I think we are concerned about the thousands of people who went through, for instance, the mines - Reeves Mine, Metachewan, Matheson - who were sort of lost to view now, and lost to view in terms of compensation as well.

A. My feeling is that it's quite worthwhile to identify high risk plants, areas, jobs and what not, and to spend particular effort in tracking down and monitoring those people. I don't feel that wholesale coverage of everyone in the province who has ever come into contact with an asbestos fiber for one day when they were a summer student will be particularly useful, given that we haven't got unlimited resources.

I think there are a number of special cases in the province - Johns-Manille, Raybestos Manhattan maybe being

A. (cont'd.) several of them, and I think we should concentrate our efforts in those cases where we can do something useful.

Q. What about some of the mining populations that were exposed to fairly high exposures?

A. I haven't got any detailed information about those people. If it should turn out that, you know, there was substantial exposure long enough ago that it would be worthwhile to track these people down now rather than ten years from now, then I think it would be worthwhile to do that.

However, if the Quebec experience is any guide, the mining population is probably at considerably lower risk than the manufacturing sector.

Q. I want to go back to the mortality study and the issue of measurements. First of all, I found it very interesting that you presented data from insurance hygienists. Is that a common practice... the insurance companies are taking hygiene surveys?

A. It apparently was years ago. I think in one of Weill's papers he mentions that there was some insurance...it may be that Johns-Manville employed an insurance company either to come in and make measurements, or as part of their requirements for insurance coverage the insurance companies insisted that some measurements be made.

Q. Is that true in other toxic substances as well? Have you run across it?

A. I have no idea. This dates back to the mid-fifties.

Q. Right. Right. The one thing you did say, Mr. Hardy was talking to you fairly extensively, how many weeks ago, about the measurements, and it is true that you submitted your measurements to the company for comments and that they didn't respond with any criticism. Is that correct?

A. I submitted my measurements to the company. They made no particular comment about any of the numbers I sent.

Q. But they did have the opportunity to comment on them?

A. Yes, they did.

Q. In your tab three, on the mortality among workers receiving compensation, how did you treat lung cancer cases before 1976, that were compensated for lung cancer?

Were they included in this study as asbestosis cases?

A. The only people I included were those whose diagnosis at the time of compensation was asbestosis.

Q. Right. And before 1976, you had to have asbestosis in order to receive compensation for lung cancer?

A. That's before my time. I'm not sure about that. What I did was, I went through all the records and anyone who had a malignancy at the time compensation was awarded was not included.

Q. Right. So that they would have been excluded?

A. They may have had asbestosis, but it didn't seem very sensible to include them in the mortality study when they had a condition at the time the award was made which was likely to kill them within several years.

Q. But which came first, the chicken or the egg? These people did have asbestosis and indeed there were ten mesothelioma cases compensated during that period that also had asbestosis.

A. Okay. The question I asked was, what happens to a man who receives compensation for asbestosis from the Compensation Board.

Q. I think my concern is only that perhaps the outcome is even underestimated because...

A. Well, you are then asking a different question -

A. (cnt'd.) what happens to a man...or essentially what you are asking is what is the life expectancy for someone with a malignancy, which is a different question.

Q. No, I'm suggesting that those people with mesothelioma that had asbestosis, we don't know that they wouldn't have received compensation for asbestosis and then die of mesothelioma.

A. I'm not aware that there were actually any individuals in that category.

Q. There were ten, in fact, according to the Workmen's Compensation brief to the Commission, that in fact had asbestosis as well as mesothelioma.

A. These individuals probably had asbestosis and then went on to die of mesothelioma, rather than the other way around, in which case they would have been included.

There was no one who had a mesothelioma at the time asbestosis was diagnosed.

Q. Can you tell us what the compensation outcome for the people that died in your study, the ones that were not a hundred percent? Do you have any idea?

A. No.

Q. Okay. The last question - I have two short questions and then a final - you indicated that the chest division does not use the ILO radiological classification. Could you tell us what classification they use for pneumoconiosis and asbestosis?

A. I believe they use a classification which is based on some South African system which is a six point scale from zero to six.

I can't tell you too much more about it other than that.

Q. Do you know how widely that classification is used in the world?

A. No, I don't, but I think the important thing about it is that it's consistent inhouse. Whether it agrees with anybody else is sort of irrelevant for compensation purposes. I think it's of some importance for epidemiological purposes. In terms of deciding whether a man has asbestosis or not, it really doesn't matter what you call the film - as long as you read it appropriately.

Q. Is Professor Ritchie on the Mesothelioma Panel?

A. I don't believe he is.

Q. The final question is in your actual background document to the standard...

A. Yes.

Q. ...and that was, on page eighty-nine you made a reference to...at the very end...this document was written when?

A. August, 1978.

Q. In 1978. And the statement on page eighty-nine is, it says:

> "It is to be hoped that over the next few years new information will appear which will enable rational, ongoing, re-evaluation of any occupational standard".

Is that happening within the Ministry of Labour?

A. I several weeks ago completed a revision of this document, which should be made available, and supported the Ministry's asbestos standards...some time next month.

Q. Next week?

A. Next month.

Q. Before or after the public meeting? That's an unfair question, I'm sorry.

A. The document is completed. It has not yet been approved for release.

Q. I'm sorry. I couldn't hear the answer with

Q. (cont'd.) my colleagues...

M. CASGRAIN: I just said it was a good question.

MISS JOLLEY: Q. No, I just couldn't hear the answer, I'm sorry.

THE WITNESS: A. There is a new document that has not yet been approved for release.

MISS JOLLEY: That's all the questions I have, thank you.

DR. DUPRE: Thank you, Miss Jolley.

Mr. McNamee? M. Casgrain?

M. CASGRAIN: Fine. I just have a couple of questions.

CROSS-EXAMINATION BY M. CASGRAIN

Q. Do you have any information as to how the Ontario Workmen's Comp determines a capacity for people with asbestosis?

A. No, I don't.

Q. Because you said something about the ILO classification as opposed to South African classification. You said something about it being irrelevant in terms of compensation purposes.

A. Yes, I think the label you put on a chest x-ray is not important. If you call it asbestosis, it doesn't matter whether you call this a code five film or a one/two film. The label, you know, the translation into English is asbestosis and the technical name you put on the actual x-ray pictures...

Q. Where, according to you, would you start saying something is asbestosis, supposing you were using the ILO classification?

A. Well, a one zero film is a possibility, a one one film is probably...but that's just on the x-ray picture.

A. (cont'd.) There are other medical conditions which look the same and you can never say that this is asbestosis or not asbestosis on the basis of the x-ray film. It is just suggestive or consistent with.

So in any scheme the pattern of shadows on the x-ray may or may not be asbestosis. If there's unoccupational exposure and there's no other medical condition which would explain it, then I think you are quite reasonable to assume that this is indeed asbestosis.

Q. You would work on the presumption basis, is that what you are saying?

A. Well, if a man is otherwise healthy and he has been working at Johns-Manville and breathing asbestos dust for thirty years, and he's got all kinds of opacities on his chest x-ray which are consistent with the textbook description of asbestosis, then I would say this is a reasonable presumption.

Q. I'm curious to know why Ontario Workmen's would use this South African classification rather than ILO?

A. It's probably done for historical reasons which I can't explain to you. They started reading films in 1929 or 1930. I imagine that they are continuing to be consistent with their practice.

But the actual reading of the film is, as I say, irrelevant - the label you put on it. You know, the coding is written down on a little green card and whether it says one one or code five isn't important as long as the man who is looking at the card knows what this means.

DR. UFFEN: Or as long as he's consistent.

THE WITNESS: Yes.

DR. UFFEN: You've said that from the beginning.

THE WITNESS: That's right. I could write down asbestosis in English or in French. I mean, it doesn't mean that

A. (cont'd.) the condition is any different.

Q. You stated a while ago in a question from Mr. Hardy that if one relied on the Quebec experience there would be less...the risk would be less in mining than in manufacturing, and I presume that when you said that you referred to Dr. McDonald's study of the Quebec chrysotile workers?

A. Yes.

Q. Do you agree with the evidence given by Dr. McDonald in respect of the level of safety in the Quebec mines, achieved in mines in Quebec?

A. I think there is no question that the health experience of the Quebec miners has been considerably better than the health experience of people working in manufacturing plants.

Q. Have you been able, from what you know so far, to reach a conclusion in respect of what the standard should be in a mine, so that you would have what Dr. McDonald calls an acceptable risk?

A. We may have in Ontario at the moment one active asbestos mine - I'm not quite sure. We may actually have none. I've paid no attention to what an appropriate standard for mining should be in Ontario.

If mining should become an industry in Ontario, I would recommend a standard in terms of a million particles per cubic foot rather than in terms of fibers, because that's what the epidemiological evidence is based upon.

Q. You are sure making my next question difficult. Nevertheless, perhaps we could explore what you just said, for a minute.

Are you saying that you would recommend from now on a reading in particles rather than in fibers?

A. In Ontario asbestos mines, yes. I think they should be both done side by side, but the control limit should be

A. (cont'd.) in terms of total dust.

Q. So should I understand that you would recommend that a reading in fibers be not the standard used? It would only be for control measures?

A. In a mine, yes.

Q. Do you have a standard you would recommend to the mines in respect of millions of particles?

A. It's irrelevant in Ontario. I don't have any suggestions to make.

Q. Well, it is relevant to the extent that there is a mine which used to operate in Ontario, which still operates. But you will leave that until the time comes?

A. If I'm called upon to suggest a standard for mines in Ontario, I will suggest one in terms of total dust.

Q. So you are then unable to tell me whether an average of two fibers per c.c. is something which is an acceptable risk in a mine?

A. I have spent virtually none of my time examining what an acceptable level for a mine should be, because that's not a problem in Ontario.

Q. What about a factory? In Ontario, using chrysotile?

A. Well, my recommendations will be released by the government within the next month. You can read what I have to say then.

Q. Well, I am somewhat curious and that being the case, if the board will permit, perhaps my question is not legal, but you know, I have listened to you and I must first congratulate you upon the fact that you are one of the witnesses who really accepted the shortcomings of any studies one had to undertake in this vast world of asbestos, but perhaps you could tell me whether your recommendation that you would like to make

Q. (cont'd.) would be that in factories one would have to have a standard based on particle rather than fiber count?

A. No, fibers.

Q. Fibers?

A. Yes.

Q. Can you then tell me what you would recommend in terms of fibers for a factory in Ontario using chrysotile?

A. I would recommend something less than two fibers.

Q. What would something less than two fibers be?

A. I can recommend only from a health effects point of view. I think that any standard has to be set taking economic and social and political considerations into account as well as the health effects considerations.

Q. Agreed.

A. From a health effects consideration, I think something under half a fiber would be appropriate.

Q. But coming back to what you would recommend taking into consideration all other elements, when you said less than two fibers, would that be one fiber point five?

A. What I have recommended is a standard less than point five.

Q. Less than point five?

A. Less than point five.

Q. That would be in fibers?

A. Yes.

Q. What would it be based on? What kind of an average? Over three-quarters of an hour, one hour, eight hours?

A. A forty hour time weighted average.

Q. A forty hour. What instrumentation do you recommend along with that?

A. Membrane filter, polarized light optical

A. (cont'd.) microscopy.

Q. Membrane filter, polarized light microscopy? Is that it?

(no audible response)

And you are recommending something less than point three fibers?

A. Point five.

Q. Less than point five. Could it be less than... could it be three?

A. Could it be three fibers?

Q. Maybe point three, that you are recommending?

A. No, I am recommending a standard less than point five.

Q. Can you be more precise than this?

A. Point three is certainly less than five, yes.

Q. How is one going to meet this if one is to make averages in terms of using...is that an average, point three? Would that be an average?

A. The Ontario government will come out with a standard September 21st, I believe. You will be able to see then what they recommend.

Q. As opposed to what you recommended?

A. What I have recommended is a standard less than point five.

Q. Did you hear the evidence of Mr. Trudeau and others as well, who testified about the fact that this is very difficult to count fibers with optical light microscopy when they are less than point five?

A. I was not present for that.

Q. Did you hear about this evidence? Do you know about it?

A. From what I read, the fifteen minute detection

A. (cont'd.) level is about point one fiber.

Q. With an optical light microscopy?

A. Yes, that is actually why NIOSH recommended point one fiber.

Q. Is it based on what you read?

A. Yes.

Q. What did you read?

A. I have read that point one fiber is the statistically-detectible level for a fifteen minute sampling period.

Q. Who wrote that?

A. I don't know. This is in the NIOSH criteria document, I believe.

As I finished telling you, my recommendations are based solely on health effects considerations. The standard that is adopted by the Ontario government and any other jurisdiction should weigh health effects, hygiene, economics, politics and social considerations.

I am not concerned with the absolute niceties of hygiene. However, from what I read, point one is certainly measurable with polarized light optical microscopy.

Q. Electron microscopy, would that help?

A. It's too expensive, I think, for routine monitoring. It can be done on a research basis.

Q. Can I come back again to this point that you made about the fact that you would recommend, for mines you would recommend particles rather than fibers, and would you tell me again why you say that?

A. Well, because there is epidemiology - McDonald being the best, probably, there is the Rubino study from Italy - which is measured in terms of particles.

Q. But if one accepts the fact that mines started counting in fibers in 1975, for instance...?

A. It doesn't matter. The studies are published in terms of particles.

Q. But what about the experience that one gathers from 1975 on? What does one do with it?

A. Well, you measure in parallel, and twenty years from now you will now what an appropriate fiber standard is for a mine.

Q. So what you are really saying is because past studies in mines were based on particles, you say you would recommend continuing to count in particles and use that as a standard for the future? Is that what you are saying?

A. Until enough time has gone by you can make an intelligent deduction about what the appropriate fiber standard should be for a mine, yes.

Q. In terms of particles? If you are going to use particles as your standard, that's what you've got to base your safety level at?

A. That's right. I'm not saying you shouldn't do fiber measurements. I think you should do both in a mine. Twenty-five years from now you'll see if anybody gets sick. You will then be able to say what the appropriate fiber standard should be to prevent those people from getting sick.

Q. Are you talking about twenty-five years from today that you would find out whether people...?

A. Yes. Twenty-five years from 1975, because that's when you started doing your fiber counts.

Q. What you are saying to us today is that it doesn't matter what one may do now in terms of having counted in fibers, we still have to wait twenty-five years from 1975 to find out whether or not a two fiber standard is safe? Is that what you are saying?

A. What I am saying is that we don't have asbestos

A. (cont'd.) mines in Ontario, and whatever you decide to do in Quebec is a Quebec jurisdictional problem.

If I were advising the Quebec government, I would advise them to set their hygiene standard for the asbestos mines in terms of particles.

Q. I think you and I agree, don't you, that the... in fact what the present recommendations of this body are going to be will affect to a great extent the Quebec producers, who produce all of the asbestos in Canada except for Cassiar and somewhere else? Don't you agree?

A. Are we talking about money or health? Certainly the impact of what this Commission decides will probably have very little effect on hygiene conditions in any Quebec mines. It may very well put people out of work, but whether the standard should be in fibers or in total dust, I think is not a problem for this Commission.

Q. Well I have the feeling that I have...my presence here has been useful to me and to my client, and they think that this Commission has been very useful to the whole industry in the whole world..just to put this in proper perspective.

DR. UFFEN: Could I ask a little question in here, following up about the possibility of a mine in Ontario?

If this Commission makes recommendations, if the Ontario government makes regulations and standards, should we not bear in mind the possibility, though, that mines could open up in Ontario within the life span of the regulations?

What I am concerned about is the possibility that it takes five years to change the regulations, and only two years to open up a mine.

THE WITNESS: Yes, well, I would insert a clause to the effect that standards for mines should probably make use of the McDonald epidemiological results, which are expressed in terms

THE WITNESS: (cont'd.) of particles.

DR. UFFEN: As an addition to the regulations? Is that what you are implying here?

THE WITNESS: I think it's very difficult to generalize from one situation to another. I think the McDonalds have done a very nice job in defining health risks in chrysotile asbestos miners.

I think one would have to make considerably less extrapolation to Ontario asbestos mines if you used their results as they presented them, than to go through the game of trying to convert from particles to fibers.

DR. UFFEN: I followed you on that part. It's just the concern about an existing standard...suppose it is in fibers per milliliter, cubic centimeter, and then someone opens a mine and they are obliged to adhere to the existing regulation in terms of fibers per c.c. when you have just given us very persuasive reasons for using particles per cubic foot.

THE WITNESS: Yes. I think if you apply the manufacturing standard to a mine, you will not be exposing miners to any risk, greater risk, than you are exposing people in the manufacturing sector to.

It may be that because of the mining process that a more relaxed standard might be suitable in the mining situation. Certainly it would seem from the Quebec mining experience that, you know, if the suggested conversion factors are appropriate, that miners and millers can be exposed to larger quantities of fibers than people in the manufacturing sector and suffer lesser health risks.

So that any manufacturing standard you apply to the mining sector would probably be conservative.

Q. Have you, in making the recommendation of something which I now believe to be less than zero five, have you

Q. (cont'd.) given any thought to monitoring and how that would be achieved?

A. Yes.

Q. How would the monitoring be achieved?

A. Exactly the same way it is currently being done in Ontario.

Q. By membrane filter reading?

A. Yes.

Q. Have you yourself carried out any experiments to in effect try and read this on a membrane filter, with light microscopy? Have you tried that? And to read in effect point three fibers? Have you ever done that?

A. I have never flown an airplane and I have never driven a train. There are lots of things I haven't done. Other experts do that.

Have you yourself done it to tell me that it's impossible?

Q. Yes, I have. I have read fibers on membrane and I must tell you if you ask me that question - and one should never ask a question of a lawyer because one gets the answer he doesn't want to have sometimes - I have several times read membrane...

A. Well, in terms of...

Q. ...it is extremely difficult to read one fiber with any certainty, and if you were to put fifteen different readers you might have a tremendous difference between them.

I also have visited factories where in effect they have experimented with reading of fibers, and have come to the conclusion that it is impossible to do any monitoring of fiber counting at the levels that you have suggested.

Does that satisfy your question?

MISS JOLLEY: Perhaps we could cross-examine you.

M. CASGRAIN: Q. The question I want to ask you is this: Have you had anyone conduct an experiment to see whether monitoring this type of standard is feasible using present technology, without going to electron microscopy?

THE WITNESS: A. What I'm concerned about is preventing men dying from cancer.

Q. I congratulate you on this, but that doesn't answer my question.

A. If you can't monitor a standard which is dangerous, then perhaps we should ban asbestos.

Q. So your suggestion is either we adopt your standard or ban asbestos if we can't monitor...

A. No, I'm not suggesting that at all. I am suggesting that my recommendation for a standard based upon health effects is a level less than point five fibers.

Q. Would you now answer my question?

A. I am not the individual who chooses a standard for Ontario.

Q. Would you now answer my question? Have you had anyone specifically, recently, try to monitor - without using electron microscopy - the reading of fibers at that level you suggested, those levels you suggested? Have you done that recently, to support your...

A. So you are saying it's impossible?

Q. No. I'm asking you whether you have ascertained whether it was feasible or not.

A. Well, as you know...

Q. Recently.

A. ...Johns-Manville measurements in their West Hill plant in the years 1976 through 1978 were routinely measuring fibers levels of point one, point two, point three.

Q. I don't know that. I don't know. I'm asking you whether you have recently caused anyone to carry out an experiment to see whether or not this was feasible using other equipment than electron microscopy?

A. No, sir, I haven't.

Q. You have not?

A. I have not, no.

Q. Then you don't know whether this is achievable or not, do you? In terms of practicality? Practical application? Do you?

A. I read books, sir, and from what I read it is feasible.

Q. Which book? You tell me which book you read that told you this was feasible?

A. The NIOSH documents, 1976 and 1980.

Q. It's a pretty wide book. Which section of that book and on what author was it based?

A. I think this line of questioning is completely irrelevant to my testimony, and I'll pass.

Q. I think I'll pass, too. There will be two of us passing.

DR. UFFEN: Can I ask a question?

In the monitoring of very low levels, do you have any views about any modifications of the procedures of the membrane filter method that would allow you to have more fibers to count... for example, measure longer, so that you get enough that your statistics would be...

THE WITNESS: Sure. The point one level is suggested for a fifteen minute measurement interval. If you count or if you sample for half an hour, or two hours, or for a four hour day, then you are going to be able to detect lower levels.

I'm not quite sure why NIOSH...actually, you know,

THE WITNESS: (cont'd.) they say point one is what is currently feasible. If you look at the tables in their document you can see that it's based on the fifteen minute sampling period.

So certainly if you select a longer sampling period - which is done - I think it's not infrequent to sample for two hours or for a work shift.

DR. UFFEN: What about a little faster rate of pumping of the air through the filter? I believe my memory tells me it's two liters per...oh, I've forgotten now...but it seemed to me rather slow, and that you could double it, maybe even triple it, without introducing too great an error, and thus get more fibers to count.

THE WITNESS: This is completely outside of my field. There is a fellow in the Ministry, Gyan Rajhans, who has written two books on sampling techniques and asbestos hygiene. He certainly would be the one to ask about things like that.

I have no technical knowledge about this.

DR. DUPRE: Can I take it, Dr. Casgrain, on the basis of your dialogue with...

M. CASGRAIN: I'm flattered, Dr. Dupre, but I am...

DR. DUPRE: My mistake. Can I take it on the basis of your dialogue with M. Casgrain, Dr. Finkelstein, that it is in part the J-M experience with measurements between 1976 and 1979 that gives you confidence about the reliability of low fiber measurements?

THE WITNESS: I'm not saying anything about the reliability. I'm saying that the levels that J-M and the Ministry both measured were...there were measurements routinely in the point one through point five range.

Reliability is another question. I think that there is probably an uncertainty easily of fifty to a hundred percent

THE WITNESS: (cont'd.) on any sample.

However, NIOSH as well has also published another document dealing with compliance assessment. They tell you how many measurements you have to make and how you assess them in order to say whether or not a particular industry or a particular factory is meeting whatever threshold or occupational standard you are having to make.

If there is a degree of uncertainty with any measurements, then you have to make more of them in order to say that they are in compliance or they are not.

If you go into a factory and there is a standard of two fibers, and on one sample you measured three point five, then you are really not justified to say that this particular factory is not in compliance with the standard.

So, you know, there is the statistical science of sampling and conclusions to be drawn from the various sampling programs.

DR. DUPRE: M. Casgrain...?

M. CASGRAIN: I have no further questions.

DR. DUPRE: No further questions?

Mr. McNamee, please?

CROSS-EXAMINATION BY MR. McNAMEE

Q. Yes. Dr. Finkelstein, on your table seven in exhibit thirty-six...pardon me, tab seven, I'm sorry, the mesothelioma deaths, you did have a number in your table of people with very short exposures and I understand that they were taken out of your mortality study because they didn't have the requisite nine years of work.

I just wonder if you might look here at numbers one, two, nine and fifteen, and all of these are extremely short exposures, in the 1948 to 1950 period.

Q. (cont'd.) I just wonder because there might be a significant fact to be taken from them that in the absence of other exposure that a very short exposure might cause mesothelioma.

Did you do anything to take out these four people and investigate what other exposures they might have to asbestos to maybe help yourself to decide whether an extremely short exposure might in fact cause mesothelioma?

A. No, I have not yet done that.

Q. Would it be possible with the data you have to maybe select those four cases and see whether...to exclude the possibility of other asbestos exposure and...?

A. Well...

Q. What I mean to say, it wouldn't be impossible to say..

A. Several of these men have been compensated by the Compensation Board, so there is an occupational history for them.

Q. Are those four people part of the...?

A. I have informed the Board of several other deaths. Presumably what they will attempt to do is to locate the widow and find out whether she wishes to initiate a claim, in which case it might be possible to obtain an occupational history.

There are some problems with confidentiality. The provincial registrar in allowing this information to be made available to me will not allow me to contact next of kin. So that...

Q. So there are some limitations imposed on the depth of your study as far as other occupational exposures and following these people through?

A. That's right.

Any man who is investigated by the Compensation Board, there is a possibility of getting a complete occupational history. For those who are not, then we are not allowed to contact next of kin.

Q. Well, these four people that I have numbered wouldn't be compensated in any event.

A. Which numbers were they?

Q. Numbers one, two, nine and fifteen. In any event, they wouldn't have been compensated because of the short employment history. Is that correct?

A. Nine and fifteen were both compensated. One, the Board is looking into, and two, the diagnosis has yet to be confirmed.

Case two, the man died in British Columbia and I've yet to obtain pathological material from this case.

Q. In the nine and fifteen the work experience would be, occupational experience would be available to you, is that correct?

A. Yes.

Q. Have you looked at that to indicate to yourself whether there are other asbestos exposures?

A. I have not yet, no.

Q. You indicated that your proposed standard is less than five point fibers. Is that just for chrysotile, or do you have a...

A. That's for all Canadian asbestos fibers, chrysotile, crocidolite and amosite.

Q. So it's one universal standard you are proposing?

A. Yes.

Q. I would understand from the way you answered M. Casgrain, that when you say less than five, that you didn't recommend a specific number less than five, you just said less than five without indicating a particular figure to the people to whom you recommended it?

A. Yes. As I said, there are other considerations

A. (cont'd.) to be taken into consideration by those individuals who will eventually propose the standard. This is just one aspect of it.

Q. I have only one further question. In table one in your mortality study, which is tab five, the C, the unexposed workers, do I understand those are the people in the rockwool plant and fiber glass and minimally-exposed people?

A. Yes, primarily. There are also a few other people who worked in the power house, and there is an individual who worked at the front gate.

Q. There was about eighty-seven of those, I believe, was there not?

A. Something like that.

Q. On all...all your observed expected are all at a factor of one, except for the very last - heart disease. Were these people also compared with the overall Ontario population and not...

A. Yes. This comparison is with the Ontario population.

MR. McNAMEE: Thank you. Those are my questions.

DR. DUPRE: Dr. Uffen?

DR. UFFEN: I have one curious thing that has come up since you were here before. We were told that in the mesothelioma cases from Quebec, a surprising amount of the fibrous mineral tremolite was found in the lungs on autopsy. In the cases that you dealt with, do you know whether any such identification was made?

THE WITNESS: I'm not aware that tissue has been analyzed for asbestos content. Professor Ritchie occasionally does fiber counts by gram block of tissue, but no sophisticated analysis for fiber identification has been done.

DR. DUPRE: Dr. Mustard?

DR. MUSTARD: A general question. You've listened to testimony from a number of constituencies about asbestos exposure and health effects, and you have substantial experience with the problems in Ontario in the Johns-Manville plant. Do you have any ideas about why the observed versus expected ratios in the cohort that you are looking at in the Johns-Manville plant are in a bigger magnitude than in many of the other studies?

Is it...I've asked you a series of questions...do you think it's just pure chance, do you think it's the nature of the plant, or do you think it's the tightness of the cohort of people that you are working with?

THE WITNESS: Yes. I think most of the other studies are diluted with short-term people. I'm currently attempting to trace shorter-term Johns-Manville employees. I would expect their health experience would be better than the longer-term people, so if I add them all together at the end I would expect the relative risks would drop for the group as a whole.

So that's part of it. This is people with nine or more years of exposure who are going to be at a much higher risk than a group which includes men with eighteen months or twenty-four months exposure.

DR. MUSTARD: If I were trying to make an estimate of the level of exposure that a worker should have, in your view would I get a more accurate assessment by working with a tight, well-defined cohort or a diluted-out cohort?

THE WITNESS: Well, if you are able to assign each individual an exposure index, then it doesn't matter. The more individuals you've got in your group the better. Very few studies are able to assign individual exposures. Selikoff's study, we have seventeen thousand men and he makes no assignment whatsoever.

THE WITNESS: (cont'd.) The only other fiber-related study in the world is Peto's from Rochdale.

So, you know, there just are very few studies where this is done at all.

DR. MUSTARD: I have the impression that the assignment of workers in the Mansville plant in Ontario is a very good relationship to their individual probability of exposure. Is that impression a fair impression to have on the basis of your review of the evidence?

THE WITNESS: Well, what I did was, I attempted to assign each individual a measure of exposure. How accurate that was is open to serious question, which is why I attempted to bracket the risk by proposing or examining what would happen if I underestimated by a factor of ten.

DR. MUSTARD: But now I would like to have your assessment of the evidence that you have heard from Mr. Peto about the tightness of these relationships. How comfortable do you feel with the relationship in terms of the Johns-Manville data and the data that you've heard about from the other studies?

THE WITNESS: Could you be a little more specific in exactly what you are asking me to compare?

DR. MUSTARD: I'm asking you a fairly general question because I have a series of concerns. Let me sort of unfold them and then maybe give you some perspective.

We've had data presented to us about life expectancy estimates of people exposed to asbestos, and obviously if one has say a hundred people with very sharp, well-defined strong exposures to asbestos, and another cohort of a thousand or two thousand people in that pool, if you make your life expectancy calculations you will get a blend of the whole mixture and you may not show very much. Whereas if you pooled a hundred and do their life expectancy estimates, you may find that they are considerably shortened.

DR. MUSTARD: (cont'd.) So those two thousand in that may have had some exposure to asbestos, but we are left with uncertainty about what they really had been exposed to.

The thing that is of concern to me is how we get a feel for that as Commissioners.

Now, the Johns-Manville plant, having seen the building from the outside and not from the inside, is a fairly well enclosed...well, it was a modern structure when it was built after the Second World War, in which the dust levels should be, I suppose, definable within that structure, fairly well.

One has the impression on listening to you and your evidence, and having heard the other evidence, that (a) you have a fairly tightly-defined cohort in terms of the intensity of the exposure they are likely to get, and a reasonably good linkage between what they did and the kind of exposure that they would get.

Some of the other material that we've heard, it seems that the cohorts are more diluted out possibly, and you mentioned that yourself, and that the tightness of the data about linking to exposures may not be as strong.

I just wonder if you have any feel for that in going through all the data. I realize it's hard to test, and I am just simply asking for your impression.

THE WITNESS: Well, when the document that I have just finished is released, you will be able to read my specific considerations, but in summary I would say there are only two cohorts in the world which enable one to make a quantitative risk estimate - that's the Johns-Manville and the Rochdale cohort.

I think all of the others have such serious problems that they can't be used at all.

DR. MUSTARD: Thank you.

DR. DUPRE: Dr. Uffen, do you wish to ask another question?

DR. UFFEN: Yes. I had forgotten this one.

You have given us some pretty clear advice on your view from the health point of view about standards. We are still faced with the problem, or I have the problem, how to set a standard for occupational exposure of short duration but possibly quite intense - the demolition industry, the telephone people who drag wires through, put cables into buildings and they may get just a brief exposure, but much greater than we have seen for a long time.

How should that be regulated?

THE WITNESS: It's a very difficult question. The Ontario government actually has excluded construction workers from their toxic substance regulations and is proposing separate regulations for the construction industry.

The problem is that by the time you get there to measure, they've already gone. So it would seem to me that the really only feasible approach is to require that these people use adequate protective equipment and sort of forget what the levels are.

Well, I retract that. Obviously if you are planning a demolition, you can arrange things such that there is a combination of protective equipment and enclosures and...

DR. UFFEN: Work practices?

THE WITNESS: Yes. Exactly.

DR. UFFEN: Specify work practices rather than some level of...

THE WITNESS: That's right. You know, it takes days to analyze the results, it takes a while to get there to measure them, and conditions have changed and the job is finished by the time all this is done.

So I think occupational standards probably only are practical in situations where you can predict the same job is going

THE WITNESS: (cont'd.) to be done six months from now as is being done today. These here-today-and-gone-tomorrow situations, you require a different sort of approach.

DR. UFFEN: Thank you.

DR. DUPRE: Dr. Finkelstein, can we go back to your tab seven for a moment, your list of mesothelioma deaths? I'm trying to test my recollection of your testimony of a couple of weeks ago.

Case ten is a case of a maintenance worker who is excluded from the material found in your table two, the mortality study?

THE WITNESS: Yes, yes.

DR. DUPRE: Now, looking at table one in the same mortality study, where you have separately recorded production workers, the sum of production and maintenance workers, and your unexposed workers, shouldn't I expect to find that case number ten represented in your mesothelioma column?

THE WITNESS: You should, but you won't, because this man's death was coded to lung cancer.

DR. DUPRE: I see.

So how then am I...okay.

THE WITNESS: You see table one is the code that Queen's Park people put on the death certificates.

DR. DUPRE: Okay, but...

THE WITNESS: Which doesn't necessarily correspond to what the actual cause of death was.

DR. DUPRE: Right. And this is a CTRC best evidence?

THE WITNESS: Yes.

DR. DUPRE: I see. Fine. Thank you very much.

One other question having to do with tab seven. You mentioned that you now have a sixteenth case?

THE WITNESS: Yes.

DR. DUPRE: Did I understand you to say that the period of employment is 1955 to 1958?

THE WITNESS: Yes.

DR. DUPRE: Three year employment?

THE WITNESS: That's right.

DR. DUPRE: So that case would not find its way into your mortality cohort, given the nine year employment qualification?

THE WITNESS: Correct.

DR. DUPRE: One other question, Dr. Finkelstein. If I could take you back to tab three, and to the table three on page 261 of tab three, where you record twenty-three observed deaths from nonmalignant respiratory disease, have you been able in the interval since you were last with us to ascertain what you told us you couldn't at the time without access to your data - namely how the twenty-three might break down as among asbestosis, versus pneumonia, versus bronchitis or emphysema and so on?

THE WITNESS: I have not done that, no.

DR. DUPRE: But it is still possible for you to do that? It's simply a matter of...

THE WITNESS: Yes, I've got the data. It's just a question of...

DR. DUPRE: I have no further questions.

Counsel, do you wish to address any final questions?

MR. LASKIN: Just one or two final questions.

EXAMINATION BY MR. LASKIN

Q. Could I just follow up on one matter that Dr. Mustard raised, and I'm just wondering whether you gave any consideration or did any calculation of loss of life expectancy in respect of any of your studies - I suppose your mortality study?

Q. (cont'd.) We've had some evidence here that that is one index of risk that one can look at, and I'm just wondering whether you either gave any consideration to doing that kind of calculation or in fact did it?

A. I gave it consideration, but I didn't do it.

Q. Can you tell us why?

A. No. We've got the computer program written, I think. We actually tried it for our silicosis study, but that just...the problem with loss of life expectancy is that it's only really a good thing to do when your whole cohort is dead, you know, because it keeps changing as time goes on and men are still alive.

You know, you can ascertain loss of life expectancy and basically the earlier in time you measure your loss of life expectancy, the larger it's going to be because, you know, the younger men have died. The next year people have lived longer and they will have lost less life expectancy.

So it's sort of a moving number that keeps running from you.

If you've got a group of men who are virtually all dead, then you can calculate a loss of life expectancy and that's going to give you a pretty reasonable assessment of what it will eventually be when they are all dead.

But for a group where most of the men are still alive, you know, every day that you calculate this number can be different from the numbers that you calculated previously.

So, similarly, you know, the mean age of death in those tables that I gave you for mesothelioma and lung cancer is going to get larger. If someone dies from mesothelioma tomorrow, he's going to be somewhat older, probably, because the cohort is getting older. Eventually, when they are all dead, the average age is going to be higher than it was for whatever day it was that I calculated that particular number.

A. (cont'd.) So it's a very slippery kind of figure to deal with.

I think it's very interesting, this kind of number, because this is what is socially important - the years of life that you've lost - but it's not a number that stays still for you. It keeps jumping around.

Do you follow it?

Q. Yes, I think I follow it. If you do it, is the figure that you ultimately get an average figure spread over the whole cohort?

A. It will be, yes, because you add up the...you take the age at death of every man in the group, you subtract that from the age at which you would have expected him to have died and you come up with a number for each man.

You then divide it by the total number of deaths and come up with an average.

Q. I suppose the trouble I'm having is that when I've seen that figure it seems to be expressed in terms of days, and for example not very many long days. Yet when you look at a table such as your table seven on mesothelioma deaths, you see that the people have actually died, seem to have had their life shortened quite considerably. So I suppose my question is, what kind of significance does one attach to that kind of figure?

A. I think that has to be assessed in context. I can give you a global answer. You've got to see how it was calculated and decide what to do with it.

All of these figures are averages in a certain sense, and you've got to decide which one best answers the question you are interested in asking.

What I've done actually in my risk calculations is that I've sort of gotten away from that completely, and I've asked the question 'how many men would die between the ages of

A. (cont'd.) forty-five and sixty'.

What a lot of people have done and what the British have done in particular, you know, what's your lifetime risk. They have taken observations based on a twenty or twenty-five year period, extrapolated it to fifty years and say, you know, X percent of the group will die.

I think that's a pretty hazardous thing to do because (a) you assume that the rates are going to stay the same, where we see from most asbestos diseases they rise as time goes on so that you are probably going to underestimate the eventual fifty year mortality.

Also, by doing that, just by saying there is an extra two percent of people who die, you ignore when they die, which I think is socially important.

Q. Just one other, one or two other questions in relation to a discussion you had with M. Casgrain, and it concerned the recommendation that you gave which you said would be less than point five, which I take it is ultimately going to be contained in some document?

A. It's sitting right here, but it's not releasable at the moment.

Q. Is it a document that is an update of a document that we've already got before us in exhibit thirty-six?

A. It's essentially a rewrite of...

Q. Is it tab two?

A. Yes.

Q. Can I ask you, and I haven't reviewed tab two in detail for a while, but can I ask you whether your judgement - not getting into specifics - has your judgement changed from the time you wrote tab two until the time you have written this present document?

A. I was very naive when I wrote tab two. I am a little bit more mature and my opinions have changed a bit.

Q. Has there been any particular evidence that you have taken into account in writing the new document that you didn't take into account in respect of tab two?

A. I think the easiest answer would be to ask you to wait and read it?

Yeah, in other words the update of the Rochdale study...

Q. By Peto?

A. Peto, and the Johns-Manville data.

Q. Is your judgement...

A. As well there is Berry's update of the asbestosis study of Rochdale.

Q. Just one final question. Has your judgement changed on the issue of fiber type between the time you wrote tab two...

A. Yes.

Q. ...and your present study?

A. I think there is very little evidence in support of differential toxicity from various fibers. I think there is good reason from a hygiene point of view to have a uniform standard for all three, since it's very difficult if not impossible to distinguish them under the optical microscope, and I think the epidemiology may be a little bit misleading. The statements that are coming out from Quebec are that chrysotile is less hazardous. I suspect that may very well be because the dose that the Quebec miners received is considerably less than what other people have received. Their lung cancer rates are much lower as well.

This is Peto's contention as well.

Q. When you give that opinion are you taking into account the mesothelioma evidence as you perceived it, as well as lung cancer evidence?

A. Well, you know, the contention is that the

A. (cont'd.) whole question is based upon mesothelioma and that chrysotile is less hazardous in terms of producing mesothelioma.

I don't accept that point of view. I don't think it's supported.

So I have chosen to recommend a uniform standard for the three major fiber types.

Q. Do you have any particular view on the gas mask workers study?

A. In what sense?

Q. In terms of fiber type and mesothelioma?

A. Well, they've had a lot of mesotheliomas with a lot of crocidolite in their lungs, and actually if you look at the ratio of mesotheliomas to excess lung cancers, you find that it's not much different from the Quebec experience.

Q. In the gas mask workers study?

A. Yes. The problem is in comparing these studies it's impossible to compare doses. All you've got are numbers of deaths. It wouldn't surprise me at all if the Quebec miners were exposed to a lot of dust, but not much of it got into their lungs because it was just too coarse.

MR. LASKIN: Thanks very much, Dr. Finkelstein. That's all, Mr. Chairman.

DR. DUPRE: I think Dr. Mustard has a question.

DR. MUSTARD: I would just like to go back to some questions that Mr. Hardy was asking of you. I guess it's particularly related to table four in tab five.

I just wanted to ask you a question about those figures that you were trying to, as I understand it, show the mortality rates against your exposures of three groups?

THE WITNESS: Yes.

DR. MUSTARD: The problem Mr. Hardy was identifying is that the rate for the lung cancer group in the heavy exposure group, C, is less that in the moderate exposure group, B.

I don't know if this is a problem or not, but putting aside for the moment the problem that physicians and pathologists will want to classify cancer in the very general terms and that mesothelioma will get tabbed as lung cancer, and your probability of sorting that out depends upon the rigor with which it's done.

But the other problem that I was wondering about that might affect that is that if you get severe fibrosis in your lungs, called asbestosis, you increase the risk of dying from respiratory disorders or from right-sided heart failure. I think one of the things that I can say with some confidence in these hearings is that death is a certainty - you only die once - which makes it very difficult to die from asbestosis and then die from lung cancer.

So that in effect if you have people with heavy exposure and your asbestos paper shows that they have a higher mortality, that the time of onset of their mortality seems to be in the timeframe where your cancer risk is appearing, have you done any calculation as to whether in that heavy exposure group they make some allowance for the fact that some people who might get lung cancer die before the lung cancer can be fully manifested because they come down with respiratory diseases which kill them? Is that a factor influencing the calculation of a heavy exposure group when you are trying to do these kinds of comparisons?

Am I making myself clear?

THE WITNESS: Yes, yes. Certainly.

It's only indirectly addressed here and, you know, I'm looking at rate so that if someone dies from something else

THE WITNESS: (cont'd.) he doesn't contribute to the denominator.

There are some statistical techniques, I think, that you can attempt to use to take competing causes of death into account. I have not yet attempted to apply them to this data.

DR. MUSTARD: No one will have done this, I guess, so it must be a problem in the heavy exposure groups that we can have competing causes of death which in a sense...

THE WITNESS: Yes, except that if you look at the all cause mortality rates, the actual number of respiratory deaths was not very large.

DR. MUSTARD: No, but...

THE WITNESS: The production workers, there were only four.

DR. MUSTARD: I guess the question I would have to ask you, is the bulk of that mortality in your heavy exposure group - which would be what I was trying to get at here - and if they are loaded in there, they have an effect.

THE WITNESS: I really can't answer that now.

You know, if anything that's going to be a smallish effect here. I can't explain this, you know. In the text I have proposed several possibilities - one, just small number fluctuations, and the other being difference in smoking habits between the groups, and the third is there is always a possibility that my exposure assignments just weren't good enough when I was classifying them.

DR. MUSTARD: But out of that discussion it seems to me there is another possibility. Let me try to state it clearly...

THE WITNESS: Yes, I understand that. I mean the extreme example is that if all the men in group C were run over by a bus, none of them would have died of lung cancer.

DR. MUSTARD: But the more important consideration for me is that if in heavy-exposure individuals the risk of getting asbestosis is increased - which is what your data shows - and if with that group there is the greatest risk of dying from the consequences of asbestosis that are nonmalignant, and if the time of onset of those deaths corresponds with the fatal curve where the upswing is in the manifestation of cancer, then you have two competing causes of death.

THE WITNESS: Yes, but if you look at table one it can only be a small factor because there is only a total of four respiratory deaths. So even if all four of them were in the group C men...

DR. MUSTARD: Table four in...?

THE WITNESS: Table one.

DR. MUSTARD: Table one in tab...?

THE WITNESS: In the same tab, the second to last row, which is inappropriately labelled 'respiratory heart disease', which is really nonmalignant respiratory disease.

That table that you've got right on top there.

DR. MUSTARD: It's interesting. I put the four plus the five in the cancer, that would change it around dramatically if the competition were real, but the numbers are small in that table as well.

THE WITNESS: Yes, but then you are assuming that all these respiratory deaths would, within this time interval, have turned into lung cancer deaths.

DR. MUSTARD: Refresh me. Is table four only the...

THE WITNESS: Production workers.

DR. MUSTARD: ...production workers.

THE WITNESS: Yes.

DR. MUSTARD: It doesn't include the...

THE WITNESS: Yes.

DR. MUSTARD: That's all I have at this time.

DR. DUPRE: Well then, counsel, can I take it that we will be adjourning now until tomorrow morning at ten o'clock, and the witness will be Dr. Allison McDonald?

MR. LASKIN: That's correct, Mr. Chairman.

DR. DUPRE: And that tomorrow's hearing will be the last hearing this month?

MR. LASKIN: That's right, and perhaps for some considerable period of time.

M. CASGRAIN: Mr. Chairman, do I understand that whether or not you finish Dr. Allison McDonald tomorrow, you not sit Friday?

DR. DUPRE: That's correct.

M. CASGRAIN: I came prepared to stay Friday. I'm not saying I didn't want to.

DR. DUPRE: And, of course, given the fact that... I believe Dr. McDonald is coming from the U.K. She is not back in Montreal yet.

MR. LASKIN: That's correct.

DR. DUPRE: So I think that certainly makes it imperative upon us to use her time wisely so that we may complete our work with her by the end of the day. So we may want to bear in mind that we will be sitting right through perhaps until at least six o'clock.

Dr. Finkelstein, may I please, sir, thank you very, very much indeed for your testimony here.

We shall now rise until ten o'clock tomorrow.

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THE INQUIRY ADJOURNED

THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht

EDWINA MACHT